XV. *Eaſy Methods of meaſuring the Diminution of Bulk, taking place upon the Mixture of common Air and nitrous Air; together with Experiments on* Platina. *By* John Ingenhouſz, *M. D. F. R. S. Phyſician to their Imperial Majeſties at* Vienna. *In a Letter to Sir* John Pringle, *Bart. P. R. S.*

TO SIR JOHN PRINGLE, BART. P. R. S.

SIR, Vienna, Nov. 3, 1775.

R. Feb. 15, 1776. SOME time ago I amuſed myſelf with ſome experiments relating to nitrous air. Having received from the learned Abbé FONTANA a copy of a pamphlet, which he publiſhed this year under the title, *Deſcrizione e uſi di alcuni ſtromenti per miſurare la ſalubrita del aria, di Felice* FONTANA. *In Firenze, l'anno* MDCCLXXV: *per Gaetano Cambiagi Stampatore granducale*, which moſt probably will already be known to you; I imitated ſome of them, and found them very uſeful for the intended purpoſe of meaſuring the quantity of air abſorbed or diminiſhed by mixing the nitrous with the common air; by which *criterion* the degree of the ſalubrity of common air may be aſcertained according to the diſcovery of Dr. PRIESTLEY. Abbé FONTANA firſt produces nitrous air in a ſeparate veſſel, and then forces it into the glaſs, or other veſſel, in which it is to remain, till a communication be opened between this veſſel and the other

other which contains common air. I found it a difficult matter to force always juſt the ſame quantity of nitrous air into the veſſel; becauſe I could never be ſure that the nitrous air had diſlodged all the common air out of it, or had diſlodged always the ſame quantity of common air. If this quantity is not always juſt the ſame, ſome variety muſt happen in every experiment; and thus an exact valuation of the quantity of air abſorbed cannot well be made. To obviate in ſome meaſure this difficulty, and to abridge the experiment by mixing ſuddenly the two airs together, I contrived the inſtrument of which I ſend you here a drawing. It is a ſtrong glaſs veſſel, nearly two inches and a half in diameter, and about as much in height: a conical figure would perhaps be better. A braſs cover, which embraces the glaſs about half an inch downwards, is cemented to it, and has a hole in its middle, correſponding with the hole in the glaſs veſſel. This hole of the braſs cover has a female ſcrew fitted to receive the male ſcrew of a braſs tube, about ſeven inches long and about an inch in diameter, terminating at one end in a male ſcrew (adapted to the abovementioned copper plate) and at the other, in a neck adapted to enter the mouth of an elaſtic gum bottle, otherwiſe called *boradchio* or *caout-chouc*, to be tied to it with a ſtrong ribbon. This braſs tube has towards each extremity an air-tight cock, by which the communication between one extremity and the other may be opened or ſhut. Between theſe two cocks, about the middle of the tube, is a ſhort lateral tube, communicating

nicating with the canal of the other tube. This lateral tube has alſo an air-tight cock, which opens or ſhuts up the communication with the long tube, and has a female ſcrew to receive the male ſcrew of another ſhort tube, which ſerves to receive a glaſs tube bent at right angles and of two feet or more in length; the diameter ſomewhat more than that of a large quill. This glaſs tube is to be divided into any number of equal parts. I uſe the inſtrument in the following manner. The elaſtic gum-bottle being well tied to the braſs tube, all the cocks ſhut, and the glaſs tube fixed to its place, I pour a certain quantity of *aqua fortis* (v. g. ℥ ß) into the glaſs veſſel, taking care that none of it touches the braſs cover: then I put into it a certain quantity of iron filings (v. g. ʒj) wrapt up in a bit of paper to prevent its being immediately corroded. This being done, I ſcrew the glaſs veſſel to the braſs tube, ſo that no air can get out. When the red fumes begin to riſe, I open the two cocks of the braſs tube, which open the communication between the glaſs veſſel and the elaſtic gum-bottle. By ſqueezing the elaſtic gum-bottle, I force the two airs to mix together. The diminution of the air is ſoon perceived by the elaſtic gum-bottle becoming flaccid. When I judge the air is as much diminiſhed as it can be, I put the extremity of the glaſs tube into a veſſel with water, and open the cock of the ſide tube: the water immediately riſes in the glaſs tube to a height proportioned to the diminution of the two airs. By repeating ſeveral times the experiment in the ſame place, I found the

riſe of the water nearly the ſame, though not ſo exactly as I could have wiſhed: the variation I aſcribed partly to the elaſtic bottle not being always of the ſame firmneſs or elaſticity, which it loſes more or leſs by ſqueezing. I contrived another method more ſimple, and perhaps more accurate, which is the following: I took a glaſs tube about two and a half feet long, and not quite a twelfth of an inch in diameter; ſo that a column of quickſilver might ſlide through the whole without diſperſing itſelf, filling always the whole cavity. I cemented to each extremity a braſs ring, that I might be able to ſhut the opening with my finger without hurting myſelf. This tube being divided into 100 equal parts, I uſed it in two different ways; *viz.* having poured ſome *aqua fortis* into a little phial, and put to it ſome filings, I thruſt the extremity of the glaſs tube, into the neck of the phial. A column of quickſilver of about an inch in length occupied that end of the glaſs tube which was in the neck of the phial. The whole was kept in ſuch a poſture that the tube was nearly in an horizontal line, the end which is put into the phial being rather the higheſt. Care was taken that the tube ſhould not touch the *aqua fortis*. The phial being filled with red fumes and the extremity of the tube ſurrounded with them, I open and ſhut alternately the oppoſite extremity of the tube, ſo as to allow the quickſilver to advance ſlowly towards the middle; as ſoon as the column of quickſilver is arrived at the middle, I take the tube out of the bottle, and ſhut each extremity with the fore-finger: thus moving the tube upwards and downwards as briſkly as can be done with a certainty of keeping both extremities

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all the while exactly ſhut. The two airs being thoroughly mixed, I put one extremity into a veſſel filled with quickſilver, and withdrawing the finger from the opening, the quickſilver riſes immediately within the tube, and ſhews by its height the exact quantity of air diminiſhed. The other method is this: I tye to the end of the ſame tube the neck of a ſmall elaſtic gum-bottle, the bottom of which is cut away: having put ſome iron filings into a little phial, filled with *aqua fortis*, I put the end of the tube within the mouth of the phial, clapping my hand faſt to the orifice of the phial, the looſe part of the elaſtic bottle, ſo that the nitrous air, riſing from the phial, muſt take its courſe through the tube. When the whole tube is filled with red fumes, I take it out, and ſhut the two extremities with my two fore-fingers. Then I put one end of the tube in a veſſel with quickſilver, and withdraw both fingers for an inſtant, to make the column of quickſilver riſe within the tube. I apply immediately both fingers; and holding the tube nearly in a horizontal direction, ſo that the extremity where the quickſilver is may be rather the higheſt, I open and ſhut at the ſame time both extremities, ſo that the column of quickſilver gradually advances towards the middle. The quickſilver advancing towards the middle, as much common air follows the quickſilver as it forces out nitrous air from the other extremity. As ſoon as the column of quickſilver is in the middle, I keep both extremities well ſhut with my fingers, and moving the tube in various ways, I force the two airs to come into mutual contact, and to mix intimately together. Then I put one extremity

extremity into a veſſel filled with quickſilver, withdraw the finger from within the quickſilver, and obſerve to what height the quickſilver riſes. It requires ſome practice to perform this experiment with dexterity.

Some time ago I got ſome ounces of fine *platina* from Spain, through the means of his excellency Count DIETRICHSTEIN, with which I made ſome experiments. Moſt writers aſſert, that a conſiderable part of the *platina* is attracted by the magnet, but not the whole of it: but by a nice inquiry I found, that every one of the particles obeyed the magnet more or leſs, except ſome tranſparent ſtony particles; and that even theſe were all magnets in themſelves; or that each particle had two poles, which I could change at pleaſure by the application of magnetical bars. Though their magnetical virtue is always much leſs than that of particles of iron, yet every one had more or leſs of it; but ſome ſo little as not to be perceived but by applying a ſtrong magnet to them when floating upon water. Beſides the flat, ſmooth, and ſhining bright particles, which are alone the true *platina*, I find two other kind of particles among them; *viz.* ſome very ſmall black particles, moſt of which are of an irregular figure, reſembling the iron ſand found in ſome parts of North America; at Teneriffe; near ſome lakes in Italy; in ſome rivers in Tranſylvania, among the gold duſt. which is taken out of them; and in many other places. Some of theſe black particles, though few in compariſon with the number of the irregular particles, are of a very regular figure; and when ſeen through a good magnifier, ſomewhat reſemble the figure of a melon.

Theſe black particles of both ſorts, I find, are attracted by the loadſtone, and have each of them two poles, though thoſe of an irregular figure have them more manifeſtly (a). The other particles are of a gold colour; having, in general, more or leſs of a paleneſs approaching to the colour of *platina*. Some of theſe gold particles have the figure of the reſt of the *platina*, differing only from them in colour, and in not being ſo bright, or as it were poliſhed. Others are irregular maſſes of indeterminate figure having generally a ſpungy appearance. The moſt part of theſe gold particles were evidently attracted by the magnet, and ſhewed upon the ſurface of the water their two diſtinct poles. Theſe gold particles being put upon a piece of charcoal, and the flame of a candle directed upon them by the blowpipe of the chemical pocket laboratory, deſcribed by GUSTAVE VON ENGESTROM, publiſhed in the Engliſh tranſlation of CROWNSTED'S *Mineralogie*, run eaſily into round balls, which have all the appearance and quality of real gold, except their being in general magnetical or having two diſtinct poles. I make no doubt but this magnetical quality is owing to ſome *platina* mixed with the gold. I could never melt a ſingle particle of true ſhining *platina* by blowing ſtrongly upon it with the blowpipe; the only change they underwent by this operation was to loſe their brightneſs and the greateſt part of their

(a) If magnetiſm is a criterion of iron, there muſt be iron in the *platina*; but if the reſt of this ſubſtance be gold, according to ſome, why ſhould not this be precipitated together with the gold added to it, by the addition of a ſolution of green vitriol to the *aqua regia* in which the two metals are diſſolved?

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magnetical virtue. Having filled a ſmall glaſs tube with that *platina*, I found each end of it attracted both poles of a compaſs indiſcriminately; but being put to a ſet of magnetical bars, it became a real magnet, having two diſtinct poles, which I could change at pleaſure. I filled another ſmall tube with *platina*, the hollow of the tube being only of ſuch a ſize, as to allow the particles of *platina* to go in freely. I ſtuck a pin in each end, and fixed the pins with ſealing wax. I directed five or ſix electrical exploſions from three very large jars through the tube; after which, I found the *platina* had acquired no polarity. By looking with a microſcope at the outſide of the tube, I found the *platina* was much changed, ſo as to appear one uninterrupted cylinder of metal, all the interſtices between each particle being quite, in appearance at leaſt, obliterated and filled with bright metal. The places which were not bright, were become of a black hue, and appeared to be parts of the *platina* not melted; which I found afterwards to be the caſe. I attempted to ſhake the particles out of the tube, but I could not ſucceed. I could only force out ſome few at the opening with a pin. I ſeparated a little bit of the tube with a file, to puſh out the cylinder of *platina*; but could not ſucceed without employing a great force: therefore I beat ſome part of the tube to pieces with a hammer, and found each particle had undergone a remarkable alteration. All of them appeared in ſeveral places to have been melted, and ſome little ones ſeemed to have been intirely in a fluid ſtate; they all adhered in lumps together ſo ſtrongly, that many of them

them could abſolutely not be rubbed aſunder between the fingers. The inſide of the tube exhibited marks of having received impreſſions of the melted metal. By comparing the ſeparated particles of this *platina* with particles not expoſed to an electrical exploſion, they were ſcarce to be known for the ſame ſubſtance. I had put ſome iron filings in a tube of the ſame ſize, and directed the ſame exploſion through it, in order to compare the effect of electricity upon it with what happened to the *platina*. I found, by looking at the outſide, ſomewhat of the ſame appearance of being melted. By cutting this tube in ſmall bits, I could eaſily puſh out the filings with a pin, which I could not do in the other caſe but with great force. The filings ſtuck together, as the particles of *platina* had done; but with leſs force. By this experiment it ſhould ſeem as if *platina* (which hitherto could never be melted by common fire by itſelf, but only in the *focus* of a very ſtrong burning glaſs, ſuch as was a little while ago made at Paris) were equally fuſible, if not more ſo than iron, by electrical fire. I was ſomewhat ſurprized to find, that the particles of *platina* taken out of the aforeſaid tube, had got a remarkably ſtronger magnetical force, being attracted by a loadſtone at a greater diſtance, and turning their poles more briſkly upon the water than before, though the whole cylinder of theſe particles, ſtill incloſed in the tube, gave no ſigns of having acquired polarity. Thus it appears, that common fire diminiſhes the magnetical virtue of *platina*, and that electrical fire increaſes it; which I thought the more pro-

bable, becauſe thoſe very particles, which had acquired by electricity their increaſed magnetical force, did loſe it again after being heated upon a piece of charcoal, which did not happen in the particles of iron. *Platina* mixed with lead was put upon an ordinary cupel in a docimaſtic furnace ſtrongly heated. When the metal came to a ſolid ſtate, it was a flat rough lump, much heavier than the crude *platina*. I put freſh lead to it, and cupelled it again as before. I repeated it ten times, when I obtained a large lump, ſomewhat leſs flat, pretty ſmooth, but not bright; of about the ſame weight as after the firſt cupellation(b). This lump did not give the leaſt ſign of magnetiſm, and even would not receive any by being applied to ſtrong magnetical bars (I forgot to try this after the firſt cupellation) and the ſubſtance was very brittle, nearly of the ſame colour as *platina*, and took a fine poliſh. If it could tend to any uſeful purpoſe, I would repeat theſe experiments oftener, to be quite ſure whether the event would be conſtantly the ſame.

Though a piece of ſoft iron attracts the two poles of a compaſs indiſcriminately, and is incapable of acquiring polarity itſelf, yet I have never been able to ſeparate a ſingle particle of the ſofteſt iron, even when I ſeparated it carefully with a flint, or other body containing no ſteel or iron, without its giving evident ſigns of two diſtinct poles when floating upon water, nay even upon paper. I could alſo never find iron filings of ever ſo ſoft

(b) I loſt the paper that contained the exact weight before and after the cupellation.

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a ſubſtance, but each particle ſeparately had evidently two poles. Such iron filings mixed with bees wax, as much as is ſufficient to keep them together, got a ſtrong polarity by being touched with magnetical bars, and had all the qualities of a magnet: the maſs is eaſily cut with a warm knife, and is very convenient for magnetical experiments, ſuch as Dr. KNIGHT made with ſimilar loadſtones made of pounded magnets. I found alſo, that each particle of thoſe granulated iron ores of Sweden, which are placed among the *mineræ ferri retractoriæ*, ſeparated iron from ſtone, and had two diſtant poles; and that a piece of the ore itſelf became a tolerable good magnet by being touched with the bars.

I am, &c.